tion that during the comparatively short period since they came under European observation they have risen from practically the lowest to a comparatively high stage of culture. Kane, who in the early 'fifties first described them, found that they possessed little iron or wood, using sledge-runners of bone and pieces of barrel-hoops as knives. They did not hunt the reindeer, and were ignorant of the use of the bow and arrow; they could not catch salmon, and did not use the kayak. These cultural deficiencies were certainly survivals of their primitive social condition. During the 'sixties, however, they learned from emigrants from the American side of Smith's Sound the art of reindeer hunting, the use of the bow and arrow, skill in salmon catching, and the mode of building kayaks and hunting from them. The leader of this party of foreigners, Kridlarssuark, has now become the legendary culture hero. Finally, in 1891, Peary began his intercourse with them, which enabled them to obtain in exchange for their fox and bear skins the finest American weapons, with the result that the rapid destruction of game will probably soon destroy their main source of livelihood. Even up to the time of Peary's first visit stone knives and axes were in use, and they used to make rude implements with cutting edges of meteoric iron, the source of which was discovered by Peary during a later expedition in 1894. Even now they make their harpoon points of iron with a head-piece of bone, and they work iron with much skill with the files they used for the older material.

A similar course of evolution may be traced in the construction of their houses. In their original home they must have used whale-ribs for the support of the roof. Wood of sufficient span being now not procurable, they have, while retaining the primitive plan, adopted a new device for supporting the roof, planned on the model of the cantilever.

With this modern and fairly advanced culture the Polar Eskimo combines many savage characteristics. He is, says Dr. Steensby, "a confirmed egoist, who knows nothing of disinterestedness. Towards his enemies he is crafty and deceitful; he does not attack them openly, but indulges in back-biting; he will not meet his deadly enemy face to face, but will shoot or harpoon him from behind." They practise a rude form of justice. One man, because he was a notorious liar, was summarily killed by two chiefs, one of whom annexed the wife of the deceased.

We have said enough to show the interest and value of this account of a little known tribe. It is illustrated by characteristic sketches, the work of an Eskimo woman, which in style closely resemble the Bushman drawings recently published under the editorship of Mr. H. Balfour.

THE BRITISH ASSOCIATION AT SHEFFIELD.

SECTION I.

PHYSIOLOGY.

Opening Address by Prof. A. B. Macallum, M.A., M.B., Ph.D., Sc.D., LL.D., F.R.S., President of the Section.

The record of investigation of the phenomena of the life of animal and vegetable cells for the last eighty years constitutes a body of knowledge which is of imposing magnitude and of surpassing interest to all who are concerned in the studies that bear on the organic world. The results won during that period will always constitute, as they do now, a worthy memorial of the intense enthusiasm of the scientific spirit which has been a distinguishing feature of the last six decades of the nineteenth century. We are to-day, in consequence of that activity, at a point of view the attainment of which could not have been predicted half a century ago.

This body of knowledge, this lore which we call cytology, is still with all this achievement in one respect an undeveloped science. It is chiefly—nay, almost wholly—concerned with the structural or morphological side of the cell, while of the functional phenomena our knowledge is only of the most general kind, and the reason is not far to seek. What little we know of the physiological side of the cell—as, for example, of cellular secretion, absorption, and nutrition—has only to a very limited extent been the outcome of observations directed to that end. It is in very great part the result of all the inferences and generalisations drawn from the data of morphological research. This knowledge is not the less valuable or the less certain because it has been so won, but simply because of its source and of the method by which we have gained it; it is of a fragmentary character, and therefore less satisfactory in our estimation.

This state of our knowledge has affected—or, to express it more explicitly, has fashioned—our concept of living matter. When we think of the cell it is idealised as a morphological element only. The functional aspect is not ignored; but we know very little about it, and we veil our ignorance by classing its manifestations as vital phenomena.

It is true that in the last twenty years, and more particularly in the last ten, we have gathered something from biochemical research. We know much concerning ferment or catalytic action, of the physical characters of colloids, of the constitution of proteins, and their synthesis in the laboratory promises to be an achievement of the near future. We are also in a position to understand a little more clearly what happens in proteins when, on decomposition in the cell, they yield the waste products, urea, and other metabolites, with carbon dioxide and water. Further, fats can be formed in the laboratory from glycerine and fatty acids, a large number of which have also been synthesised, and a very large majority of the sugars of the aldohexose type have been built up from simpler compounds. These facts indicate that some of the results of the activity of animal and vegetable cells may be paralleled in the laboratory, but that is as far as the resemblance extends. The methods of the laboratory are not as yet those of nature. In the formation of carbohydrates, for example, the chlorophyll-holding cell makes use of processes of the most speedy and effective character, but nothing of these is known to us except that they are quite unlike the processes the laboratory employs in the artificial synthesis of carbohydrates. Nature works unerringly, unfalteringly, with an amazing economy of material and energy, while "our laboratory syntheses are but roundabout ways to the waste sink."

In consequence, it is customary to regard living matter as unique—*sui generis*, as it were, without an analogue or parallel in the inorganic world—and the secrets involved in its actions and activities as insoluble enigmas. Impelled by this view, there are those, also, who postulate as an explanation for all these manifestations the intervention in so-called living matter of a force otherwise and elsewhere unknown, biotic or vital, the action of which is directed, according to the character of the structure through which it operates, to the production of the phenomena in question. Living protoplasm is, in this view, but a mask and a medium for action of the unknown force.

This is an old doctrine, but it has again made headway in recent years owing to the reaction from the enthusiasm which came from the belief that the application of the known laws of physics and chemistry in the study of living matter would explain all its mysteries. A quarter of a century ago hopes were high that the solution of these problems would soon be found in a more profound comprehension of the laws of the physical world. Since then there has been an extraordinary increase in our knowledge of the structure and of the products of the activity of living matter without a corresponding increase in knowledge of the processes involved. The obscurity still involving the latter appears all the greater because of the high lights thrown on the former. Despair, in consequence, has taken the place of hope with some, and the action of a mysterious force is invoked to explain a mystery.

It may be admitted that our methods of investigation are very inadequate, and that our knowledge of the laws of matter, seemingly comprehensive, is not at present profound enough to enable us to solve all the problems involved in the vital phenomena. The greatest factor in the difficulty of their solution, however, has been the fact that there has been a great lack of investigators specially trained, not only in biology, but also in physics and chemistry, for the very purpose of attacking intelligently such problems. The biologists, for want of such a wide training, have emphasised the morphological aspect and the readily observable phenomena of living matter; while the physicist and chemist, knowing little of the morphology of the cell and of its vital manifestations, have been unable to apply satisfactorily the principles of their sciences to an understanding of its processes. The high degree of specialism which certain departments of biology has in recent years developed has made that difficulty greater than it was.

It must also be said that in some instances in which the physicist and chemist attempted to aid in the solution of biological problems the result, on the whole, has not been quite satisfactory. In, for example, the phenomena of osmosis, the application of Arrhenius's theory of ionisation and van 't Hoff's gas theory of solutions promised at first to explain all the processes and the results of diffusion through animal membranes. These theories were supported by such an array of facts from the side of physics and physical chemistry that there appeared to be no question whatever regarding their universal validity, and their application in the study of biological phenomena was urged with acclaim by physical chemists and eagerly welcomed by physiologists. The result in all cases was not what was expected. Diffusion of solutes, according to the theories, should, if the membrane is permeable to them, always be from the fluid where their concentration is high to that in which it is low. This appears to happen in a number of instances in the case of living membranes—or, at least, we may assume that it occurs—but in one signal instance, at least, the very reverse normally obtains. In the kidney, membranes formed of cells constituting the lining of the glomeruli and the renal tubules separate the urine, as it is being formed, from the blood plasma and the lymph circulating through the kidney. Though the excreted fluid is derived from the plasma and lymph, it is usually of much greater osmotic concentration than the latter.

It may be urged that this and other discrepancies are explained by the distribution (or partition) coefficient of the solutes responsible for the greater concentration of the product of excretion, these solutes being more soluble in the excreted medium than in the blood plasma, and distributing or diffusing themselves accordingly. If such a principle is applicable here as an explanation, it may be quite as much so in other physiological cases in which the results are supposedly due only to the forces postulated in the theories of van 't Hoff and Arrhenius. Whether this be so or not, the central fact remains that the enthusiastic hopes with which the theories were applied by physiologists and biologists in the explanation of certain vital phenomena have not been wholly realised.

The result has been a reaction amongst physiologists and biologists which has not been the least contributory of all the causes that have led to the present revival of vitalism.

Another difficulty in accounting for the vital phenomena has been due, until recently, to a lack of knowledge of the physical and chemical properties of colloids and colloidal "solutions." The importance of this knowledge consists in the fact that protoplasm, "the physical basis" of life, consists mainly of colloids and water. Until eleven years ago, what was known regarding colloids was derived chiefly from the researches of Graham (1851–62), Ljubavin (1889), Barus and Schneider (1891), and Linder and Picton (1892–7), who were the pioneers in this line. In 1899 were published the observations of Hardy, through whose investigations very great progress in our knowledge of colloids was made. In 1903 came the invention of the ultramicroscope by Siedentopf and Zsigmondy, by which the suspension character of colloid material in its so-called "solutions" was visually demonstrated. During the last seven years a host of workers have by their investigations greatly extended our knowledge of the physical and chemical properties of colloids, and now the science of Collochemistry bids fair, the more it develops, to play a very important part in all studies bearing on the constitution and properties of living matter.

Then, also, there are the phenomena of surface tension. This force, the nature of which was first indicated by Segner in 1751, and described with more detail by Young in 1804 and Laplace in 1806 in the expositions of their theories of capillarity, was first in 1869 only casually suggested as a factor in vital processes by Engelmann. Since the latter date and until 1892, when Bütschli published his observations on protoplasmic movement, no serious effort was made to utilise the principle of this force in the explanation of vital phenomena. Even to-day, when we know more of the laws of surface tension, it is only introduced as an incidental factor in speculations regarding the origin of protoplasmic movement and muscular contraction, and yet it is, as I shall maintain later on in this address, the most powerful, the most important of all the forces concerned in the life of animal and vegetable cells.

It may be gathered from all that I have advanced here that the chief defect in biological research has been, and is, the failure to apply thoroughly the laws of the physical world in the explanation of vital phenomena. Because of this too much emphasis is placed on the division that is made between the biological and the physical sciences. This division is very largely an artificial one, and it will in all probability be maintained eventually only as a convenience in the classification of the sciences. The biologist and physiologist have to deal with problems in which a wide range of knowledge is necessary for their adequate treatment; and, if the individual investigator has not a very extensive training in the physical sciences, it is impossible for him to have at his command all the facts bearing on the subject of his research, unless the problem involved be a very narrow one. The lack of this wide knowledge of the physical sciences tends to specialism, and, as the specialism is ever growing, it will produce a serious situation eventually, for it will develop a condition in the scientific world in which coordination of effort and a broad outlook will be much more difficult than is the case now.

This growing defect in the biological sciences can only be lessened by the insistence of those in charge of advanced courses in biological and physiological laboratories that only those whose training is of a very wide character should be allowed to take up research. It is, perhaps, futile to expect that such a rule will ever be enforced, for in the keen competition between universities for young teachers who have made some reputation for original investigation there may not be too close a scrutiny of the qualifications of those who offer themselves for post-graduate courses. There is, further, the difficulty that the heads of scientific departments are not desirous of limiting the output of new knowledge from their laboratories by insisting on the wider training for the men of science who are in the process of developing as students of research.

It is perhaps true, also, that there still remains a great deal unobserved or unrecorded in the fields of biology, physiology, and biochemistry, in the investigation of all of which a broad training is not specially required to give good service; and that, further, this condition will obtain for one or two decades still. It is quite as certain, however, that the returns from such service will tend to diminish in number and value, and, if the coming generation of workers is not recruited from a systematically and broadly trained class of students, a period of comparative sterility may supervene.

As it is to-day, there are few who devote themselves to the direct study of the chemical and physical properties of the cell, the fundamental unit of living matter. There are, of course, many who are concerned with the morphology of the cell, and who employ in their studies the methods of hardening and staining which have been of very great service in revealing the structural as well as the superficial chemical properties of the cell. On the facts so gained views are based which deal with the chemistry of the cell, and which are more or less widely accepted, but the results and generalisations drawn from them give us but little insight into the chemical constitution of the cell. We recognise in the morphologists' chromatin a substance which has only in a most general way an individuality, while the inclusions in the nucleus and the cytoplasm, on the distinction by staining of which great emphasis is laid, can only in a most superficial way be classified chemically.

The results of digestion experiments on the cell structures are also open to objection. The action of pepsin and hydrochloric acid must depend very largely on the accessibility of the material the character of which is to be determined. If there are membranes protecting cellular elements, pepsin, which is a colloid, if it diffuses at all, must in some cases, at least, penetrate them with difficulty. In Spirogyra, for example, the external membrane, formed of a thick layer of cellulose, is impermeable to pepsin, but not to the acid, and, in consequence, the changes which occur in it during peptic digestion are due to the acid alone. Even in the cell the periphery of which is not protected by a membrane, the insoluble colloid material at the surface serves as a barrier to the free entrance of the pepsin. It is, however, more particularly in the action on the nucleus and its contents that peptic digestion fails to give results which can be regarded as free from objection. Here is a membrane which during life serves to keep out of the nucleus, not only all inorganic salts, but also all organic compounds, except chiefly those of the class of nucleo-proteins. That such a membrane may, when the organism is dead, be permeable to pepsin is at least open to question, and in consequence what we see in the nucleus after the cell has been acted on by pepsin and hydrochloric acid cannot be adduced as evidence of its chemical or even of its morphological character.

The results of digestive experiments on cells are, therefore, misleading. What may from them appear as nucleo-protein may be anything but that, while, if the pepsin penetrates as readily as the acid, there should be left, not nucleo-protein, but pure nucleic acid, which should not stain at all.

The objections which I now urge against the conclusions drawn from the results of digestion experiments have developed out of my own observations on yeast cells, diatoms, Spirogyra, and especially the blue-green algæ. The latter are, as is Spirogyra, encased in a membrane which is an effective barrier to all colloids. When, therefore, threads of Oscillaria are subjected to the action of artificial gastric juice, a certain diminution in volume is observed owing to the dissolving power of the hydrochloric acid, and an alteration of the staining power of certain structures is found to obtain; but the pepsin has nothing to do with these, as may be determined by examination of control preparations treated with a solution of hydrochloric acid alone.

It is thus seen how slender is our knowledge of the chemistry of cells derived from staining methods and from digestion experiments. That, however, has not been the worst result of our confidence in our methods. It has led cytologists to rely on these methods alone, to leave undeveloped others which might have thrown great light on the chemical constitution of the cell, and which might have enabled us to understand a little more clearly the causation of some of the vital phenomena.

It was the futility of some of the old methods that led me, twenty years ago, to attack the chemistry of the cell from what appeared to me a correctly chemical point of view. It seemed to me then, and it appears as true now, that a diligent search for decisive chemical reactions would yield results of the very greatest importance. In the interval I have been able to accomplish only a small fraction of what I hoped to do, but I think the results have justified the view that, if there had been many investigators in this line instead of only a very few, the science of Cytochemistry would play a larger part in the solution of the problems of cell physiology than it does now.

The methods and the results are, as I have said, meagre, but they show distinctly indeed that the inorganic salts are not diffused uniformly throughout the cell, that in vegetable cells they are rigidly localised, while in animal cells, except those devoted to absorption and excretion, they are confined to specified areas in the cell. Their localisation, except in the case of inorganic salts of iron, is not due to the formation of precipitates, but rather to a condition which is the result of the action of surface tension. This seems to me to be the only explanation for the remarkable distribution, for example, of potash salts in vegetable cells. We know that, except in the chloroplatinate of potassium and in the hexanitrite of potassium, sodium and cobalt, potassium salts form no precipitates; and yet, in the cytoplasm of vegetable cells, the potassium is so localised at a few points as to appear at first as if it were in the form of a precipitate. In normal active cells of Spirogyra it is massed along the edge of the chromotophor, while in the mesophylic cells of leaves it is condensed in masses of the cytoplasm, which are by no means conspicuous in ordinary preparations of these cells.

This effect of surface tension in localising the distribution of inorganic salts at points in the cytoplasm would explain the distribution of potassium in motor structures. In striated muscle the element is abundant in amount, and is confined to the dim bands in the normal conditions. In Vorticella, apart from a minute quantity present at a point in the cytoplasm, it is found in very noticeable amounts in the contractile stalk, while in the holotrichate infusoria (Paramæcium) it is in very intimate association with the basal elements of the cilia in the ectosarc. This, indeed, would seem to indicate that the distribution of the potassium is closely associated with contraction, and, therefore, with the production of energy in contractile tissues. The condensation of potassium at a point may, of course, be a result of a combination with portions of the cytoplasm, but we have no knowledge of the occurrence of such compounds; and, further, the presence of such does not explain anything or account for the liberation of energy in motor contraction. On the other hand, the action of surface tension would explain, not only the localisation of the potassium, but also the liberation of the energy.

In vessels holding fluids, the latter, in relation to surface tension, have two surfaces, one free, in contact with the air, and known as the air-water surface, the other that in contact with the wall of the containing vessel (glass). In the latter the tension is lower than in the former. When an inorganic compound—a salt, for example—is dissolved in the fluid it increases the tension at the air-water surface, but its dilution is much greater here than in any other part of the fluid, while at the other surface its concentration is greatest. In the latter case the condition is of the nature of adsorption. The condensation on that portion of the surface where the tension is least is responsible for what we find when a solution of a coloured salt, as, *e.g.*, potassium permanganate, is driven through a layer of dry sand. If the latter is of some considerable thickness, the fluid as it passes out is colourless. The air-solution surface tension is higher than the tension of each of the solution-sand surfaces, on which, therefore, the permanganate condenses or is adsorbed. The same phenomenon is observed when a long strip of filter paper is allowed to hang with its lower end in contact with a moderately dilute solution of a copper salt. The solution is imbibed by the filter paper, and it ascends a certain distance in a couple of minutes, when it may be found that the uppermost portion of the moist area is free from even a trace of copper salt.

If, on the other hand, an organic compound—as, for instance, one of the bile salts—instead of an inorganic compound is dissolved in the fluid, the surface tension of the air-water surface is reduced, and in consequence the bile salt is concentrated at that surface, while in the remainder of the fluid, and particularly in that portion of it in contact with the wall of the vessel, the concentration is reduced.

The distribution of a salt in such a fluid, whether it lowers surface tension or increases it, is due to the action of a law which may be expressed in words to the effect that the concentration in a system is so adjusted as to reduce the energy at any point to a minimum.

Our knowledge of this action of inorganic and organic substances on the surface tension in a fluid, and of the differences in their concentrations throughout the latter, was contained in the results of the observations on gas mixtures by J. Willard Gibbs, published in 1878. The principle as applied to solutions was independently discovered by J. J. Thomson in 1887. It is known as the Gibbs' principle, although the current enunciations of it contain the more extended observations of Thomson. As formulated usually it is more briefly given, and its essential points may be rendered in the statement *that when a substance on solution in a fluid lowers the surface tension of the latter, the concentration of the solute is greater in the surface layer than elsewhere in the solution; but when the substance dissolved raises the surface tension of the fluid, the concentration of the solute is least in the surface layers of the solution.*

It is thus seen how in a system like that of a drop of water with different contact surfaces the surface tension is affected, and how this alters the distribution of solutes. It is further to be noted that for most organic solutes the action in this respect is the very reverse of that of inorganic salts. Consequently, in a living cell which contains both inorganic and organic solutes, and in which there are portions of different composition and density, the equilibrium may be subject to disturbance constantly through an alteration of the surface tension at any point. Such a disturbance may be found in a drop of an emulsion of olive oil and potassium carbonate in the well-known experiments of Bütschli. When the emulsion is appropriately prepared, a minute drop of it, after it is surrounded with water, will creep under the cover-glass in an amœboid fashion for hours, and the movement will be more marked and rapid when the temperature is raised to 40° to 50° C. All the phenomena manifested are due to a lowering of the surface tension at a point on the surface, as a result of which there is protrusion there of the contents of the drop, accompanied, Bütschli holds, by streaming cyclic currents in the remainder of the mass.

Surface tension also, according to J. Traube, is all-important in osmosis, and he holds that it is the solution pressure (*Haftdruck*) of a substance which determines the velocity of the osmotic movement and the direction and force of the osmotic pressure. The solution pressure of a substance is measured by the effect that substance exercises when dissolved on the surface tension of its solution, or, to put it in Traube's own way, the more a substance lowers or raises the surface tension of a solvent (water), the less or greater is the solution pressure (*Haftdruck*) of that substance. This solution pressure, Traube further holds, is the only force controlling osmosis through a membrane, and he rejects completely the bombardment effect on the septum postulated in the van 't Hoff theory of osmosis.

The question as to the nature of the factors concerned in osmosis must remain undecided until the facts have been more fully studied from the physiological point of view, but enough is now known to indicate that surface tension plays at least a part in it, and the omission of all consideration of it as a factor is not by any means a negligible defect in the van 't Hoff theory of osmosis.

The occurrence of variations in surface tension in the individual cells of an organ or tissue is difficult to demonstrate directly. We have no methods for that purpose, and, in consequence, one must depend on indirect ways to reveal whether such variations exist. The most effective of these is to determine the distribution of organic solutes and of inorganic salts in the cell. The demonstration of the former is at present difficult, or even in some cases impossible. The occurrence of soaps, which are amongst the most effective agents in lowering surface tension, may be revealed without difficulty microchemically, as may also neutral fats, but we have as yet no delicate microchemical tests for sugars, urea, and other nitrogenous metabolites, and in consequence the part they play, if any, in altering the surface tension in different kinds of cells, is unknown. Further research may, however, result in discovering methods of revealing their occurrence microchemically in the cell. We are in a like difficulty with regard to sodium, the distribution of which we can determine microchemically in its chief compounds, the chloride and phosphate, only after the exclusion of potassium, calcium, and magnesium. We have, on the other hand, very sensitive reactions for potassium, iron, calcium, haloid chlorine, and phosphoric acid, and with methods based on these reactions it is possible to localise the majority of the inorganic elements which occur in the living cell.

By the use of these methods we can indirectly determine the occurrence of differences in surface tension in a cell. This determination is based on the deduction from the Gibbs-Thomson principle that, where in a cell an inorganic element or compound is concentrated, the surface tension at the point is lower than it is elsewhere in the cell. If, for example, it is concentrated on one wall of a cell, the surface tension there is less than on the remaining surfaces or walls of the cell. The thickness of this layer must vary with the osmotic concentration in the cell, with the specific composition of the colloid material of the cytoplasm and with the activity of the cell, but it should not exceed a few hundredths of a milimetre (0·02–0·04 mm.), while it might be very much less in an animal cell the greatest diameter of which does not exceed $20\ \mu$.

Numerous examples of such localisation may be observed in the confervoid protophyta. In Ulothrix, ordinarily, there is usually a remarkable condensation of the potassium at the ends of the cell on each transverse wall. The surface tension, on the basis of the deduction from the Gibbs-Thomson principle, should be, in all these cases, high on the lateral walls and low on those surfaces adjoining the transverse septa.

The use of this deduction may be extended. There are in cells various inclusions the composition of which gives them a different surface tension from that prevailing in the external limiting area of the cell. Further, the limiting portion of the cytoplasm in contact with these inclusions must have surface tension also. When, therefore, we find by microchemical means that a condensation of an inorganic element or compound obtains immediately within or without an inclusion, we may conclude that there, as compared with the external surface of the cell, the surface tension is low. It may be urged that the condensation is due to adsorption only; but this objection cannot hold, for in the Gibbs-Thomson phenomena the localisation of the solute at a part of the surface as the result of high tension elsewhere of the solution is, in all probability, due to adsorption, and is indeed so regarded.

It is in this way that we can explain the remarkable localisation of potassium in the cytoplasm at the margins of the chromatophor in Spirogyra, and also the extraordinary quantities of potassium held in or on the inclusions in the mesophyllic cells of leaves. In infusoria (Vorticella, Paramœcium) the potassium present, apart from that in the stalk or ectosarc, is confined to one or more small granules or masses in the cytoplasm.

How important a factor this is in clearing the active portion of the cytoplasm of compounds which might hamper its action, a little consideration will show. In plants, very large quantities of salts are carried to the leaves by the sap from the roots, and among these salts those of potassium are the most abundant as a rule. Reaching the leaves, these salts do not return, and in consequence during the functional life of the leaves they accumulate in the mesophyllic cells in very large quantities, which, if they were not localised as described in the cell, would affect the whole cytoplasm and alter its action.

Enough has been advanced here to indicate that surface tension is not a minor feature in cell life. I would go even farther than this, and venture to say that the energy evolved in muscular contraction, that also involved in secretion and excretion, the force concerned in the phenomena of nuclear and cell division, and that force also engaged by the nerve cell in the production of a nerve impulse, are but manifestations of surface tension. On this view the living cell is but a machine, an engine, for transforming potential into kinetic and other forms of energy through or by changes in its surface energy.

To present an ample defence of all the parts of the thesis just advanced is more than I propose to do in this address. That would take more time than is customarily allowed on such an occasion, and I have, in consequence, decided to confine my observations to outlines of the points as specified.

It is not a new view that surface tension is the source of the muscular contraction. As already stated, the first to apply the explanation of this force as a factor in cellular movement was Engelmann in 1869, who advanced the view that those changes in shape of cells which are classed as contractile are all due to that force which is concerned in the rounding of a drop of fluid. The same view was expressed by Rindfleisch in 1880, and by Berthold

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[1] See Freundlich, "Kapillarchemie," p. 50, 1909.

in 1886, who explained the protoplasmic streaming in cells as arising in local changes of surface tension between the fluid plasma and the cell sap, but he held that the movement and streaming of Amœbæ and Plasmodiæ are not to be referred to the same causes as operate in the protoplasmic streaming in plant cells. Quincke in 1888 applied the principle of surface tension in explaining all protoplasmic movement. In his view the force operates, as in the distribution of a drop of oil on water, in spreading protoplasm, which contains oils and soaps, over surfaces in which the tension is greater, and as soap is constantly being formed, the layer containing it, having a low tension on the surface in contact with water, will as constantly keep moving, and as a result pull the protoplasm with it. The movement of the latter thus generated will be continuous, and constitute protoplasmic streaming. In a similar way Bütschli explains the movement of a drop of soap emulsion, the layer of soap at a point on the surface of the spherule dissolving in the water and causing there a low tension and a streaming of the water from that point over the surface of the drop. This produces a corresponding movement in the drop at its periphery and a return central or axial stream directed to the point on the surface where the solution of the soap occurred and where now a protrusion of the mass takes place resembling a pseudopodium. In this manner, Bütschli holds, the contractile movements of Amœbæ are brought about. In these the chylema or fluid of the foam-like structure in the protoplasm is alkaline, it contains fatty acids, and, in consequence, soaps are present which, through rupture of the superficial vesicles of the foam-like structure at a point, are discharged on the free surface and produce there the diminution of surface tension that calls forth currents, internal and external, like those which occur in the case of the drop of oil emulsion.

The first to suggest that surface tension is a factor in muscular contraction was D'Arsonval, but it was Imbert who, in 1897, directly applied the principle in explanation of the contractility of smooth and striated muscle fibre. In his view the primary conditions are different in the former from what obtain in the latter. In smooth muscle fibre the extension is determined, not by any force inside it, but by external force such as may distend the organ (intestine, bladder, and arteries) in the wall of which it is found. The "stimulus" which causes the contraction increases the surface tension between the surface of the fibre and the surrounding fluid, and this of itself has the effect of making the fibre tend to become more spherical or shorter and thicker, which change in shape does occur during contraction. He did not, however, explain how the excitation altered the surface tension, except to say that its effect on surface tension is like that of electricity, with which the nerve impulse presents some analogy. In striated fibre, on the other hand, the discs constituting the light and dim bands have each a longitudinal diameter which is an effect of its surface tension, and this causes extension of the fibre during rest. When a nerve impulse reaches the fibre the surface tension of the discs is altered, and there results a deformation of each involving a shortening of its longitudinal axis, and thus a shortening of the whole fibre.

According to Bernstein, in both smooth and striated muscle fibre there is, in addition to surface tension, an elastic force residing in the material composing the fibre which, according to the conditions, sometimes opposes and sometimes assists the surface tension. The result is that in the muscle fibre at rest the surface must exceed somewhat that of the fibre in contraction. In both conditions the sum of the two forces, surface tension and elasticity, must be zero. In contraction the surface tension increases, and with it the elasticity also. Taken as a whole, this would not explain the large force generated in contraction, for the energy liberated would be the product of the surface tension and the amount representing the diminution of the surface due to the contraction. As the latter is very small the product is much below the amount of energy in the form of work done actually manifested. To get over this difficulty, Bernstein postulates that in muscle fibres, whether smooth or striated, there are fibrils surrounded by sarcoplasma, and that each fibril is formed of a number of cylinders or biaxial ellipsoids singly disposed in the course of the fibril, but separated from each other by elastic material and surrounded by sarcoplasma. Between the ellipsoids and the sarcoplasma there is considerable surface tension which prevents mixture of the substances constituting both. The excitation through the nerve impulse causes an increase of surface tension in these ellipsoids, and they become more spherical. In consequence, the decrease in surface of all the ellipsoids constituting a fibril is much greater than if the fibril were to be affected as an individual unit only by an increase of surface tension, and thus the surface energy developed would be correspondingly greater. The ellipsoids, Bernstein explains, are not to be confused with the discs, singly and doubly refractive in striated fibre; for these, he holds, are not concerned in the generation of the contraction, but with the processes that make for rapidity of contraction. The extension of a muscle after contraction is due to the elastic reaction of the substance between the ellipsoids in the fibrils. Bernstein further holds that fibrils of this character occur in the protoplasm of Amœbæ, in the stalk of Vorticella, and in the ectoplasma of Stentor, and this explains their contractility.

It may be said in criticism of Bernstein's view that his ellipsoids are from their very nature non-demonstrable structures, and, therefore, must always remain as postulated elements only. Further, it may be pointed out that he attributes too small a part to surface tension in the lengthening of the fibre after contraction, and that the elasticity which muscle appears to possess is, in the last analysis, but a result of its surface tension.

As regards Quincke's explanation of protoplasmic movement and streaming, as well as of muscular contraction, Bütschli has shown that it is based on a mistaken view of the structure of the cell in Chara and other plant forms in which protoplasmic streaming occurs. Bütschli's own hypothesis, however, is defective in that it postulates a current in the fluid medium just outside the Amœba and backward over its surface, the existence of which Berthold denies, and Bütschli himself has been unable to demonstrate, even with the aid of fine carmine powder in the fluid. He did, indeed, observe a streaming in the water about a creeping Pelomyxa, but the current was in the opposite direction to that demanded by his hypothesis. Further, his failure to demonstrate the occurrence of the postulated backflow in the water about the contracting or moving mass of an Amœba or a Pelomyxa makes it difficult to accept the hypothesis he advanced to explain that backflow, namely, that rupture of peripheral vesicles (*Waben*) of the protoplasm occurs with a consequent discharge of their contents (proteins, oils, and soaps) into the surrounding fluid. Surface tension, further, on this hypothesis, would be an uncertain and wasteful factor in the life of the cell. On *a priori* grounds, also, it would seem improbable that this force should be generated outside instead of inside the cell.

One common defect of all these views is that they made only a limited application of the principle of surface tension. This was because some of its phenomena were unknown, and especially those illustrating the Gibbs-Thomson principle. With its aid and with the knowledge of the distribution of inorganic constituents in animal and vegetable cells that microchemistry gives us we can make a more extended application of surface tension as a factor in cellular life than was possible ten years ago.

In regard to muscle fibre this is particularly true, and microchemistry has been of considerable service here. From the analyses of the inorganic constituents of striated muscle in vertebrates made by J. Katz and others we know that potassium is extraordinarily abundant therein, ranging from three and a half in the dog to more than fourteen times in the pike the amount of sodium present. How the potassium salt is distributed in the fibre was unknown before 1904, in which year, by the use of a method, which I had discovered, of demonstrating the potassium microchemically, the element was found localised in the dim bands. Later and more extended observations suggested that in the dim band itself, when the muscle fibre is at rest, the potassium is not uniformly distributed, and it was found to be the case in the wing muscles of certain of the Insecta—as, for example, the scavenger beetles—in which the bands are broad and conspicuous enough to permit ready observation on this score. In these the potassium salt was found to be localised in the zones of each dim band adjacent to each light band. Subsequently Miss M. L. Menten, working in my laboratory and using the same microchemical method, found the potassium similarly limited in its distribution in the muscle fibres of a number of other insects. She determined, also, that the chlorides and phosphates have a like distribution in these structures, and it is consequently probable that sodium, calcium, and magnesium have the same localisation.

Macdonald has also made investigations on the distribution of potassium in the muscle fibre of the frog, crab, and lobster, using for this purpose the hexanitrite reagent. He holds, as a result of his observations, that the element in the uncontracted fibril is limited to the sarcoplasm in the immediate neighbourhood of the singly refractive substance, while it is abundantly present in the central portion of each sarcomere of the contracted fibril—that is, in the doubly refractive material. I am not inclined to question the former point, as I have not investigated the microchemistry of the muscle in the crab and lobster, and my only criticism would be directed against placing too great reliance on the results obtained in the case of frog's muscle. The latter is only very slowly penetrated by the hexanitrite reagent, and, apparently because of this, alterations in the distribution of the salts occur; and, as I have observed, the potassium may be limited to the dim bands of one part of the contracted fibre and may be found in the light bands of another part of the same. In the wing muscles of insects in the uncontracted condition such disconcerting results are not so readily obtained, owing, it would seem, to the readiness with which the fibrils may be isolated and the almost immediate penetration of them by the reagent. Here there is no doubt about the occurrence of the element in the zones of the dim band immediately adjacent to the light bands.

Whether the potassium in the resting fibre is in the sarcoplasm or in the sarcostyle I would hesitate to say. It may be as Macdonald claims; but I find it difficult to apply in microchemical studies of muscle fibre the concepts of its more minute structure gained from merely stained preparations. Because of this difficulty I have refrained from using here, as localising designations, other expressions than "light bands" and "dim bands." The latter undoubtedly include some sarcoplasm, but in the case of the resting fibre I am certain only of the presence of potassium, as described, in the dim band regarded as an individual part, and not as a composite structure.

Now, on applying the Gibbs-Thomson principle enunciated above, this distribution would seem to indicate that in the dim band of a fibril the surface tension is greatest on its lateral walls, in consequence of which the potassium salts are concentrated in the vicinity of the remaining surfaces, *i.e.* those limiting the light bands. This explanation would seem to be confirmed by the observations I made on the contracted fibrils of the wing muscles of a scavenger beetle. In these the potassium was found uniformly distributed throughout each dim band, which, instead of being cylindrical in shape as in the resting element, is provided with a convexly curved lateral wall, and therefore with a smaller surface than the mass of the dim band has when at rest. This contour suggests that the surface tension on the lateral wall is lessened to an amount below that of either terminal surface, followed by a redistribution of the potassium salt to restore the equilibrium thus disturbed. The consequent shortening of the dim bands of the fibrils would account for the contraction of the muscle.

How the surface tension of the lateral wall of the dim band is lessened in contraction is a question which can only be answered after much more is known of the nature of the nerve impulse as it reaches the muscle fibril, and of the part played by the energy set free in the combustion process in the dim bands. It may be that electrical polarisation, as a result of the arrival of the nerve impulse, develops on the surface of the lateral wall, and as a consequence of which its surface tension is diminished. The energy so lost appears as work, and it is replaced by energy, one may suppose, derived from the combustion of the material in the dim band. In this case the disturbance of surface tension would be primary, while the combustion process would be secondary, in the order of time. In support of this explanation may be cited the fact that the current of action in muscle precedes in time the contraction itself—that is, the electrical response of the stimulus occurs in the latent period and immediately before the contraction begins.

It may, however, be postulated, on the other hand, that the chemical changes occur in those parts of the dim band immediately adjacent to the light bands, and as a result the tension of the terminal surfaces may be increased, this resulting in the shortening of the longitudinal axis of the dim band and the displacement laterally of the contents. This would imply that the energy of muscle contraction comes primarily from that set free in the combustion process, and not indirectly as involved in the former explanation.

Whatever may be the cause of the alteration in surface tension, there would seem to be no question of the latter. The very alteration in shape of the dim band in contraction makes it imperative to believe that surface tension is concerned. The redistribution of the potassium which takes place as described in the contracting fibrils of the wing muscles of the scavenger beetle can be explained in no other way than through the alteration of surface tension.

In the smooth muscle fibre potassium is also present and in close association throughout with the membrane. When a fresh preparation of smooth muscle is treated so as to demonstrate the presence of potassium, the latter is shown in the form of a granular precipitate of hexanitrite of sodium, potassium, and cobalt in the cement substance between the membranes of the fibres. In the smooth muscle fibres in the walls of the arteries in the frog the precipitate in the cement material is abundant, and its disposition suggests that it plays some part in the *rôle* of contraction. Inside of the membrane potassium occurs, but in very minute quantities, which, with the cobalt sulphide method, gives a just perceptible dark shade to the cytoplasm as a whole. Microchemical tests for the chlorides and phosphates indicate that the cytoplasm is almost wholly free from them, and consequently there is very little inorganic material inside of the fibre. Chlorides and phosphates, but more particularly the former, are abundant in the cement material, and their localisation here would seem to indicate that the potassium of the same distribution is combined chiefly as chloride.

In smooth muscle fibre, then, the potassium is distributed very differently from what it is in striated fibre, and on first thought it seemed difficult to postulate that the contraction could be due to alterations of surface tension. This, however, would appear to be the most feasible explanation, for the potassium salts in the cement substance might be supposed to shift their position under the influence of electrical force so as to reach the interior of the membranes of the fibres, in which case the surface tension of the latter would be immediately increased, and the fibre itself would in consequence at once begin to contract. The slowness with which this shifting into, or absorption by, the membrane of the potassium salts would take place would also account for the long latent period of contraction in smooth muscle.

It is of interest here to note that the potassium ions have the highest ionic mobility (transport number) of all the elements of the kationic class, except hydrogen, which are found to occur in connection with living matter. Its value in this respect is half again as great as that of sodium, one-eighth greater than that of calcium and one-seventh greater than that of magnesium. This high migration velocity of potassium ions would make the element of special service in rapid changes of surface tension.

Loew has pointed out that potassium in the condensation processes of the synthesis of organic compounds has a catalytic value different from that of sodium. For example, ethyl aldehyde is condensed with potassium salts to aldol, with sodium salts to crotonic aldehyde (Kopf and Michael). Potassium is, but sodium is not, effective in the condensation of carbon monoxide. When phenol is fused with potassium salts condensation products like diphenol are produced, but when sodium salts are used the products are dioxybenzol and phloroglucin (Barth). It is, therefore, not improbable that potassium, along with those properties which come from its ionic mobility, has a special value in the metabolism of the dim bands of striated muscle fibre and in the condensation synthesis which characterise the chromatophors of protophyta (Spirogyra, Zygnema).

With the use of this method of determining differences in surface tension in cells it is possible, in some cases at least, to ascertain whether this force plays a part in both secretion and excretion, and evidence in favour of this view can be found in the pancreatic cells of the rabbit, guinea-pig, and in the renal cells of the frog. In the pancreatic cells there is an extraordinary condensation of potassium salts in the cytoplasm of each cell adjacent to the lumen of the tubule, and during all the phases of activity—except, it would appear, that of the so-called "resting stage"—potassium salts occur in, and are wholly confined to, this part of each cell. It is difficult to say whether they pass into the lumen with the secretion and their place taken by more from the blood-stream and lymph, but the important point is that the condensation of potassium salts immediately adjacent to the lumen seems to indicate a lessened surface tension on the lumen surface of the cell.

According to Stoklasa,[1] the pancreas of the pig is much richer in potassium than in sodium, the dried material containing 2·09 per cent. of potassium and 0·28 per cent. of sodium, while the values for the dried material of ox muscle are, as he determined them, 1·82 and 0·26 per cent. respectively. It is significant that in the pancreas this large amount of potassium should be localised as described.

In the renal cells of vertebrates there is usually a considerable amount of potassium salts distributed throughout the cytoplasm. These cells are always active in the elimination of the element from the blood, and it is in consequence not possible to determine whether there are differences in surface tension in them. Under certain conditions, however, these can be demonstrated. In the frogs which have been kept in the laboratory tanks throughout the winter, and in the blood of which the inorganic salts have been, because of the long period of inanition, reduced to almost hypotonic proportions, the renal cells are very largely free from potassium. When it is present it is usually diffused throughout the cytoplasm. If now a few cubic centimetres of a decinormal solution of potassium chloride be injected into the dorsal lymph sacs of one of these frogs, and after twenty minutes the animal is killed, appropriate treatment, with the cobalt reagent, of a thin section of the fresh kidney made by the carbon dioxide freezing method, reveals in the cells of certain of the tubules a condensation of potassium salts in the cytoplasm immediately adjacent to the wall of the lumen. There is also a very slight diffuse reaction throughout the remainder of the cytoplasm, except in that part immediately adjacent to the external boundary of the tubule. In these cells the potassium injected into the lymph circulation is being excreted, and the condensation of the element at or near the surface of the lumen is evidence that there the tension is less than at the other extremity of the cell.

These facts are in their significance in line with some observations that I have made on the absorption of soluble salts by the intestinal mucosa in the guinea-pig. When the "peptonate" of iron was administered in the food of the animal it was not unusual to find that in the epithelial cells of the villi the iron salt was distributed through the cytoplasm, but its concentration, as a rule, was greatest in the cytoplasm adjacent to the inner surface of the cell, from which it diffused into the underlying tissue. Here also, inferentially, surface tension is lower than elsewhere in the cell.

It would perhaps be unwise to form final conclusions at this stage in the progress of the investigation of the subject, but the results so far gained tempt one to adopt as a working hypothesis *that in the secreting or the excreting cell lower surface tension exists at its secreting or excreting surface than at any other point on the cell surface.* How this low surface tension is caused or maintained it is impossible to say, but, whatever the solution of the question may be, it is important to note that we must postulate the participation of this force in renal excretion in order to explain the formation of urines of high concentration. These have a high osmotic pressure, as measured by the depression of the freezing point, while the osmotic pressure of the blood plasma determined in the same way is low. On the principle of osmosis alone, as it is currently understood, this result is inexplicable, for the kinetic energy, as required in the gas theory of solutions, should not be greater, though it might be less, in the urine than in the blood. It is manifest that in the formation of concentrated urines energy is expended. We know also from the investigations of Barcroft and Brodie that the kidney during diuresis absorbs much more oxygen per gram weight than the body generally, and that, assuming it is used in the combustion of a proteid, a very large amount of energy is set free, very much more, indeed, than is necessary. It has also been observed that a portion of the energy set free is found in a higher temperature in the excretion than obtains in the blood itself circulating through the kidney. This large expenditure of energy is, probably, a result of the physiological adaptation of the principle of the "factor of safety," which, as Meltzer has pointed out, occurs in other organs of the body.

In cell and nuclear division surface tension operates as a force, the action of which cannot be completely understood until we know more of the part played by the centrosomes and centrosphere. That this force takes part in cell reproduction has already been suggested by Brailsford Robertson. He has devised an ingenious experiment to illustrate its action. If a thread moistened with a solution of a base is laid across a drop of oil in which is dissolved some free fatty acid, the drop divides along the line of the thread. When the latter is moistened with soap the drop divides in the same way and in the same plane. The soap formed in one case and present in the other, it is explained, lowers the surface tension in the equatorial plane of the drop, and this diminution results in streaming movement away from that plane which bring about the division. He suggests that in cell division there is a liberation of soaps in the plane of division which set up streaming movements from that plane towards the poles, and terminating in the division of the cytoplasm of the cell.

I have observed in the cells of Zygnema about to divide a remarkable condensation of potassium in the plane of division. In the "resting" cell of this Alga the potassium is, as a rule, more abundant in the cytoplasm near the transverse walls of the thread, and only traces of the element are to be found along the line of future division of the cell. But immediately after division has taken place the potassium is concentrated in the plane of division. This would seem to indicate that surface tension in the plane of division is, as postulated by the deduction from the Gibbs-Thomson principle, lower than it is on the longitudinal surface, and lower, especially, than it is on the previously formed transverse septa of the thread.

One must not, however, draw from this the conclusion that in all dividing cells surface tension is lower in the plane of division than it is elsewhere on the surface of the dividing structure. All that it means is that in the dividing cell of Zygnema the condition already exists along the plane of division, which subsequently makes for low surface tension in the cell membrane immediately adjacent to each transverse septum in the confervoid thread. If the evidence of low surface tension vanished immediately after division was complete, then it might be held that it determined the division. As it is, the low surface tension in this case is the result, and not the cause, of the division.

This conclusion is corroborated by the results of observations on the cells of the ovules of Lilium and Tulipa. The potassium salts in these are found condensed in minute masses throughout the cytoplasm. When division is about to begin the salts are shifted to the peripheral zone of the cytoplasm, and when the nuclear membrane disappears not a trace of potassium is now found in the neighbourhood of the free chromosomes, a condition which continues until after nuclear division is complete. The absence of potassium, the most abundant basic element in the cytoplasm, would indicate that soaps are not present, and appropriate treatment of such cells, hardened in formaline only, with scarlet red demonstrates that fats, including lecithins, are absent also. This would seem to show that high instead of low surface tension prevails about the nucleus during division. During the "resting" condition of the nucleus this high tension is maintained, for, except in very rare cases, and these of doubtful character, there is no condensation of inorganic salts in the neighbourhood or on the surface of the nuclear membrane. It is also to be noted that the nucleus, with exceptions, the majority of which are found in the Protozoa, is of spherical shape, which also postulates that high surface tension obtains either in the cytoplasmic layer about the nucleus or in the nuclear membrane itself. It may also be suggested that high surface tension, and not the physical impermeability of the nuclear membrane, is the reason why the nucleus is, as I have often stated, wholly free from inorganic constituents.

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[1] Stoklasa gave the values in K_2O and Na_2O.

It does not follow from all this that surface tension has nothing to do with cell division. If, as Brailsford Robertson holds, surface tension is lowered in the plane of division, then the internal streaming movement of the cytoplasm of each half of the cell should be towards that plane, and, in consequence, not separation, but fusion of the two halves would result. The lipoids and soaps would, indeed, spread superficially on the two parts from the equatorial plane towards the two poles, and, according to the Gibbs-Thomson principle, they would not distribute themselves through the cytoplasm in the plane of division, except as a result of the formation of a septum in that plane. In other words, the septum has first to exist in order to allow the soaps and lipoids to distribute themselves in a streaming movement over its two faces. In Brailsford Robertson's experiment this septum is provided in the thread. If, on the other hand, surface tension is higher about the nucleus in and immediately adjacent to the future plane of division, then constriction of the nucleus in that plane will take place accompanied or preceded by an internal streaming movement in each half towards its pole, and a consequent traction effect on the chromosomes which are thus removed from the equatorial plane. When nuclear division is complete, then a higher surface tension on the cell itself limited to the plane of division would bring about there a separation of the two halves, a consequent condensation on each side of that plane of the substances producing the low tension elsewhere, and thereby also the formation of the two membranes in that plane.

In support of this explanation of the action of surface tension as a factor in division I have endeavoured to ascertain if, as a result of the Gibbs-Thomson principle, there is a condensation of potassium salts in the cytoplasm at the poles of a dividing cell, that is, where surface tension, according to my view, is low. The difficulty one meets here is that, in the higher plant forms, cells preparing to divide appear to be much less rich in potassium than those in the "resting" stage, and under this condition it is not easy to get unambiguous results, while in animal cells potassium may even in the resting cell be very minute in quantity, as, for example, in Vorticella, in which, apart from the contractile stalk, it is limited to one or two minute flecks in the cytoplasm. Instances of potassium-holding cells undergoing division are, however, found in the spermatogonia of higher vertebrates (rabbit, guinea-pig), and in these the potassium is gathered in the form of a minute and thin cap-like layer at each pole of the dividing cell.

This of itself would appear to show that surface tension is less in the neighbourhood of the poles than at the equator of the dividing cell; but I am not inclined to regard the fact as conclusive, and a very large number of observations to that end must be made before certainty can be attained. I am, nevertheless, convinced that it is only in this way that we can finally determine whether differences of surface tension in dividing cells account, as I believe they do, for all the phenomena of cell division. The difficulties to be encountered in such an investigation are, as experience has shown me, much greater than are to be overcome in efforts to study surface tension in cells under other conditions, but I am in hopes that what I am now advancing will influence a number of workers to take up research in microchemistry along this line.

I must now discuss surface tension in nerve cells and nerve fibres. I have stated earlier in this address that I hold that the force concerned in the production of the nerve impulse by the nerve cell is surface tension. The very fact that in the repair of a divided nerve fibre the renewal of the peripheral portion of the axon occurs through a movement—a flowing outward, as it were—of the soft colloid material from the central portion of the divided fibre is, in itself, a strong indication that surface tension is low here and high on the cell body itself. This fact does not stand alone. I pointed out six years ago that potassium salt is abundant along the course of the axon and apparently on its exterior surface, while it is present but in traces in the nerve cell itself. In the latter chlorides also are present only in traces, and therefore sodium, if present, is there in more minute quantities, while haloid chlorine is abundant in the axon. Macdonald has also made observations as to the occurrence of potassium along the course of the axon, and has in the main confirmed mine. We differ only as to mode of the distribution of the element in the axon, and the manner in which it is held in the substance of the latter; but, whichever of the two views may be correct, it does not affect what I am now advancing. Extensive condensation or adsorption of potassium salts in or along the course of the axon, while the nerve cell itself is very largely free from them, can have but one explanation on the basis of the Gibbs-Thomson principle, and that explanation is that surface tension on the nerve cell itself must be high while it is low on or in its axon.

The conclusions that follow from this are not far to seek. We know that an electrical displacement or disturbance of ever so slight a character occurring at a point on the surface of a drop lowers correspondingly the surface tension at that point. What a nerve impulse fundamentally involves we are not certain, but we do know that it is always accompanied by, if not constituted of, a change of electrical potential, which is as rapidly transmitted as is the impulse. When this change of potential is transmitted along an axon through its synaptic terminals to another cell, the surface tension of the latter must be lowered to a degree corresponding to the magnitude of the electrical disturbance produced, and, in consequence, a slight displacement of the potassium ions would occur at each point in succession along the course of its axon. This displacement of the ions as it proceeded would produce a change of electrical potential, and thus account for the current of action. The displacement of the ions in the axon would last as long as the alteration of surface tension which gave rise to it, and this would comprehend not more than a very minute fraction of a second. Consequently, many such variations in the surface tension of the body of the nerve cell would occur in a second; and, as the physical change concerned would involve only the very surface layer of the cell, a minimum of fatigue would result in the cell, while little or none would develop in the axon.

It may be pointed out that in medullated nerve fibres the lipoid-holding sheath, in close contact as it is with the axon, must of necessity maintain on the course of the latter a surface tension low as compared with that on the nerve cell itself, which, as the synaptic relations of other nerve cells with it postulate, is not closely invested with an enveloping membrane. In non-medullated nerve fibres the simple enveloping sheath may function in the same manner, and probably, if it is not rich in lipoid material, in a less marked degree.

What further is involved in all this, what other conclusions follow from these observations, I must leave unexplained. It suffices that I have indicated the main points of the subject, the philosophical significance of which will appear to those who will pursue it beyond the point where I leave it.

In bringing this address to a close, I am well aware of the fact that my treatment of the subjects discussed has not been as adequate as their character would warrant.

The position which I occupy imposes limits, and there enters also the personal factor to account in part for the failure to achieve the result at which I aimed. But there is, besides, the idea that in applying the laws of surface tension in the explanation of vital phenomena I am proceeding along a path into the unknown which has been as yet only in a most general way marked out by pioneer investigators, and in consequence, to avoid mistakes, I have been constrained to exercise caution, and to repress the desire to make larger ventures from the imperfectly beaten main road. Perhaps, after all, I may have fallen into error, and I must therefore be prepared to recall or to revise some of the views which I have advanced here should they ultimately be found wanting. That, however, as I reassure myself, is the true attitude to take. It is a far cry to certainty. As Duclaux has aptly put it, the reason why Science advances is that it is never sure of anything. Thus I justify my effort of to-day.

Notwithstanding this inadequate treatment of the subject of surface tension in relation to cellular processes, I hope I have made it in some measure clear that the same force which shapes the raindrop is an all-important factor in the causation of vital phenomena. Some of the latter may not thereby be explained. We do not as yet know all that is concerned in the physical state of solutions. The fact, ascertained by Rona and Michaelis, that certain sugars, which neither lower nor appreciably raise surface tension in their solutions, condense or are adsorbed on the surface of a solution system, is an indication that there are at least some problems with a bearing on vital phenomena yet to solve. Nevertheless, what we have gained from our knowledge of the laws of surface tension constitutes a distinct step in advance, and a more extended application of the Gibbs-Thomson principle may throw light on the causation of other vital phenomena. To that end a greatly developed science of microchemistry is necessary. This should supply the stimulus to enthusiasm in the search for reactions that will enable us to locate with great precision in the living cell the constituents, inorganic and organic, which affect its physical state and thereby influence its activity.

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SECTION K.

BOTANY.

OPENING ADDRESS BY PROF. JAMES W. H. TRAIL, M.A., M.D., F.R.S., PRESIDENT OF THE SECTION.

THE honour conferred in the election to be President for the year of the Botanical Section of the British Association imposes the duty of preparing an address. I trust that my selection of a subject will not be attributed by anyone to a want of appreciation of the worth and importance of certain sides of botanical research to which I shall have less occasion to refer. These have been eloquently supported by former Presidents, and I take this opportunity to express the thanks I owe for the benefit received from their contributions to the advancement of the science of botany. They have told us of the advance in departments of which they could speak as leaders in research, and I do not venture to follow in their steps. My subject is from a field in which I have often experienced the hindrances of which I shall have to speak, both in personal work and still more as a teacher of students, familiar with the many difficulties that impede the path of those who would gladly give of their best, but find the difficulties for a time almost insurmountable, and who are too frequently unable to spare the time or labour to allow of their undertaking scientific investigations that they might well accomplish, and in which they would find keen pleasure under other conditions. Those whose tastes lie in the direction of studying plants in the field rather than in the laboratory are apt to find themselves hampered seriously if they seek to become acquainted with the plants of their own vicinity; and, if they wish to undertake investigations in the hope of doing what they can to advance botanical science, they may find it scarcely possible to ascertain what has been already done and recorded by others.

For a time the knowledge of plants was too much confined to the ability to name them according to the system in vogue and to a knowledge of their uses, real or imagined. The undue importance attached to this side of the study, even by so great a leader as Linnæus, naturally led to a reaction as the value of other aspects of botany came to be realised, and as improvements in the instruments and methods of research opened up new fields of study. The science has gained much by the reaction; but there is danger of swinging to the other extreme and of failing to recognise the need to become well acquainted with plants in their natural surroundings. The opportunities for study in the laboratory are so great and so much more under control, and the materials are so abundant and of so much interest, that there is for many botanists a temptation to limit themselves to such work, or at least to regard work in the field as subordinate to it and of little value. It is scarcely necessary to point out that each side is insufficient alone. Yet some find more pleasure in the one side, and do well to make it their chief study; while they should recognise the value of the other also, and learn from it.

It is especially on behalf of the work in the field that I now wish to plead. There are few paths more likely to prove attractive to most students. The study of the plants in their natural environments will lead to an understanding of their nature as living beings, of their relations to one another and to other environments, of the stimuli to which they respond, and of the struggle for existence that results in the survival of certain forms and the disappearance of others. In this way also will be gained a conception of the true meaning and place of classification as an indispensable instrument for accurate determination and record, and not as an end in itself. To one that has once gained a true insight into the pleasure and worth of such studies, collections made for the sake of mere possession or lists of species discovered in a locality will not suffice. Many questions will arise which will prove a constant source of new interest. From such studies a deep and growing love for botany has in not a few cases arisen.

The British flora has interested me for upwards of forty years, and has occupied much of my attention during that time—not only as desirous to aid by my own efforts to extend our knowledge of it, but also, as a teacher, seeking to assist my students to become able to do their parts also, and making use of the materials within reach to enable me to help them. Thus our present knowledge of the plants of our own country has become known to me, and the difficulties of acquiring that knowledge have also become known through both my own experience and those of my students. The nature of the hindrances and difficulties that at present bar the way has also become familiar, as well as the steps to be taken to clear some of them away and to make the path less difficult to those who come after us; and I have also gained a fairly good acquaintance with the means at the command of students of the floras of other countries, so as to have a standard for comparison in the estimate to be formed of the condition of matters in our own country.

In how far is the present provision for the study of the flora of the British Islands sufficient and satisfactory? I venture to hope that the subject will be regarded as among those for the consideration of which the British Association was formed, and that a favourable view will be taken of the conclusions which I take this opportunity to lay before you. What, then, is the present provision for the study of our plants? Since the days of Morrison and Ray there have been many workers, especially during the past century; and an extensive literature has grown up, in the form both of books and of papers, the latter more or less comprehensive, in the scientific journals and in the transactions of societies. These papers contain much that is of great value; but, owing to the absence of any classified index, most of the information in it is beyond the reach of anyone, except at the expenditure of much time and labour. The constantly increasing accumulation of new publications makes the need for a classified index always more urgent; for the mass of literature is at present one of the greatest obstacles to the undertaking of new investigations, because of the uncertainty whether they may not have been already undertaken and overlooked through want of time or opportunity to search the mass exhaustively.

While the early writers of descriptive floras sought to include every species of plant known to occur in Britain, this has not been attempted during the past seventy or eighty years, and instead of one great work we now have monographs of the greater groups, such as Babington's "Manual" and Hooker's "Student's Flora" of the vascular plants, Braithwaite's "Mossflora," &c. Local floras still, in a good many cases, aim at including all plants known to grow apparently wild in the districts to which they refer; but they are often little more than lists of species and varieties and of localities in which these have been found. In some, however, there are descriptions of new forms and notes of general value, which are apt to be overlooked because of the place in which they appear.

The early works were necessarily not critical in their treatment of closely allied species and varieties, but they are valuable as giving evidence of what plants were supposed to be native in England when they were published. Even the works that were issued after Linnæus had established the binominal nomenclature for a time related almost wholly to England. Sibbald in "Scotia Illustrata" (1684) enumerated the plants believed by him to be native in Scotland, and of those then cultivated. Between his book and Lightfoot's "Flora Scotia," published in 1777, very little relating to the flora of Scotland appeared. Irish plants were still later in being carefully studied.

The floras of Hudson, Withering, Lightfoot, and Smith, all of which include all species of known British plants, follow the Linnæan classification and nomenclature in so far as the authors were able to identify the Linnæan species in the British flora. "English Botany," begun in 1795, with plates by Sowerby and text by Smith, was a work of the first rank in its aim of figuring all British plants and in the excellence of the plates; but it shared the defect of certain other great floras in the plates being prepared and issued as the plants could be procured, and thus being without order. Its cost also necessarily put it beyond the reach of most botanists, except those that had the advantage of access to it in some large library. A second edition, issued at a lower price, and with the plants arranged on the Linnæan system, was inferior to the first, in the plates being only partially coloured and in having the text much curtailed. The so-called third edition of the "English Botany," issued 1868–86, is a new work so far as the text is concerned, that being the work of Dr. Boswell Syme, who made it worthily representative of its subject; but the plates, with few exceptions, are reissues of those of the first edition, less perfect as impressions and far less carefully coloured; and this applies with still greater force to a reissue of the third edition a few years ago. This edition, moreover, included only the vascular plants and Characeæ. As this is the only large and fully illustrated British flora that has been attempted, it is almost needless to add that in this respect provision for the study of the flora of our islands is far behind that of certain other countries, and very notably behind that made in the "Flora danica."

Turning next to the provision of less costly aids to the study of British plants, we have manuals of most of the larger groups. The vascular plants are treated of in numerous works, including a considerable number of illustrated books in recent years, inexpensive but insufficient for any but the most elementary students. Fitch's outline illustrations to Bentham's "Handbook to the British Flora," supplemented by W. G. Smith, were issued in a separate volume in 1887, which is still the best for use in the inexpensive works of this kind. Babington's "Manual," on its first appearance in 1843, was gladly welcomed as embodying the result of careful and continued researches by its author into the relations of British plants to their nearest relatives on the Continent of Europe; and each successive issue up to the eighth in 1881 received the careful revision of the author, and contained additions and modifications. In 1904 a ninth edition was edited, after the author's death, by H. and J. Groves; but, though the editors included notes left by Prof. Babington prepared for a new edition, they were "unable to make alterations in the treatment of some of the critical genera which might perhaps have been desirable." The "Student's Flora of the British Islands," by Sir J. D. Hooker, issued in 1870, took the place of the well-known "British Flora" (1830, and in subsequent editions until the eighth in 1860, the last three being issued in collaboration by Sir W. J. Hooker and Prof. Walker-Arnott). The third edition of the "Student's Flora" appeared in 1884, and there has been none since. Mr. F. N. Williams's "Prodromus Floræ Britannicæ," begun in 1901, of which less than one-half has yet appeared, though a work of much value and authority, is scarcely calculated for the assistance of the ordinary student; and Mr. Druce's new edition of Hayward's "Botanist's Pocket Book" "is intended merely to enable the botanist in the field to name his specimens approximately, and to refresh the memory of the more advanced worker." In all the books that are intended for the use of British botanists, apart from one or two recently issued local floras, the classification is still that in use in the middle of last century, even to the extent in the most of them of retaining Coniferæ as a division of Dicotyledones. Apart from this, the critical study of British plants has led to the detection of numerous previously unobserved and unnamed forms, which find no place in the "Student's Flora," and are only in part noticed in the recent edition of the "Manual."

The "Lists" of vascular plants of the British flora that have recently been issued by Messrs. Rendle and Britten, by Mr. Druce, and as the tenth edition of the "London Catalogue of British Plants," are all important documents for the study of the British flora; but they illustrate very forcibly certain of the difficulties that beset the path of the student eager to gain a knowledge of the plants of his native land. In these lists he finds it scarcely possible to gain a clear idea of how far the species and varieties of the one correspond with those of the other, owing to the diversities of the names employed. It would be a great boon to others, as well as to students, were a full synonymic list prepared to show clearly the equivalence of the names where those for the same species or variety differ in the different lists and manuals. Probably in time an agreement will be generally arrived at regarding the names to be accepted, but that desirable consummation seems hardly yet in sight. Meantime, the most useful step seems to be to show in how far there is agreement in fact under the different names.

Among the Cryptogams certain groups have fared better than the higher plants as regards both their later treatment and their more adequate illustration by modern methods and standards. Several works of great value have dealt with the mosses, the latest being Braithwaite's "British Moss-flora," completed in 1899. The Sphagna were also treated by Braithwaite in 1880, and are to be the subject of a monograph in the Ray Society's series. The liverworts have been the subject also of several monographs, of which Pearson's is the fullest.

Among the Thallophyta, certain groups have been more satisfactorily treated than others—*e.g.* the Discomycetes, the Uredineæ and Ustilagineæ, the Myxomycetes, and certain others among the fungi, and the Desmidiaceæ among the algæ; but the Thallophyta as a whole are much in need of thorough revision to place them on a footing either satisfactory or comparable to their treatment in other countries.

Of the Thallophyta, many more of the smaller species will probably be discovered within our islands when close search is made, if we may judge by the much more numerous forms already recorded in certain groups abroad, and which almost certainly exist here also; but among the higher plants it is not likely that many additional species will be discovered as native, yet even among these some will probably be found. It is, however, rather in the direction of fuller investigation of the distribution and tendencies to variation within our islands that results of interest are likely to be obtained.

The labours of H. C. Watson gave a very great stimulus to the study of the distribution of the flora in England and Scotland, and the work he set on foot has been taken up and much extended by numerous botanists in all parts of the British Islands. It is largely owing to such work and to the critical study of the flora necessary for its prosecution that so many additions have been made to the forms previously known as British. Many local works have been issued in recent years, often of a very high standard of excellence. Besides these larger works, scientific periodicals and transactions of field clubs and other societies teem with records, some of them very brief, while others are of such size and compass that they might have been issued as separate books. A few of both the books and papers are little more than mere lists of names of species and varieties observed in a locality during a brief visit; but usually there is an attempt at least to distinguish the native or well-established aliens from the mere casuals, if these are mentioned at all. In respect of aliens or plants that owe their presence in a district to man's aid, intentional or involuntary, their treatment is on no settled basis. Every flora admits without question species that are certainly of alien origin, even such weeds of cultivated ground as disappear when cultivation is given up, as may be verified in too many localities in some parts of our country. Yet other species are not admitted, though they may be met with here and there well established, and at least as likely to perpetuate their species in the new home as are some native species.

Comparatively few writers seek to analyse the floras of the districts treated of with a view to determine whence each species came and how, its relation to man, whether assisted by him in its arrival directly or indirectly, whether favoured or harmfully affected by him, its relations to its environment—especially to other species of plants and to animals, and other questions that suggest themselves when such inquiries are entered on. It is very desirable that a careful and exhaustive revision of the British flora should be made on these and similar lines. In such a revision it is not less desirable that each species should be represented by a good series of specimens, and that these should be compared with similar series from other localities within our islands, and from those countries from which it is believed that the species originally was sprung. Such careful comparison would probably supply important evidence of forms being evolved in the new environments, differing to a recognisable degree from the ancestral types, and tending to become more marked in the more distant and longer isolated localities. An excellent example of this is afforded by the productive results of the very careful investigation of the Shetland flora by the late Mr. W. H. Beeby.

Within recent years excellent work has been done in the study of plant associations, but the reports on these studies are dispersed in various journals (often not botanical), and are apt to be overlooked by, or to remain unknown to, many to whom they would be helpful. The same is true in large measure of the very valuable reports of work done on plant-remains from peat-mosses, from lake deposits, and from other recent geological formations, researches that have cast such light on the past history of many species as British plants, and have proved their long abode in this country. Mr. Clement Reid's "Origin of the British Flora," though published in 1899, has already (by the work of himself and others) been largely added to, and the rate of progress is likely to become still more rapid. Among the fruits and seeds recorded from inter-glacial and even from pre-glacial deposits are some the presence of which could scarcely have been anticipated, *e.g. Hypecoum procumbens*, in Suffolk. Some of the colonists, or aliens now almost confined to ground under cultivation, have been recorded from deposits that suggest an early immigration into the British Islands. While much remains to be discovered, it is desirable that what is already established should find a place in the manuals of British botany.

Apart from the descriptive and topographical works and papers on our flora, there is a serious lack of information gained from the study of our British plants. Although a few types have received fuller study, we have little to compare with the work done in other countries on the structure and histology of our plants, on the effects of environment, on their relations to other species and to animals, and on other aspects of the science to which attention should be directed. On these matters, as on a good many others, we gain most of what information can be had, not from British sources, but from the literature of other countries, though it is not wise to assume that what is true elsewhere is equally true here. It is as well, perhaps, that for the present such subjects should find scanty reference in the manuals in ordinary use; but, when trustworthy information has been gained within the British Islands, under the conditions prevailing here, these topics should certainly not be passed over in silence. Students of the British flora have as yet no such works of reference as Raunkjaer's book on the Monocotyledons of Denmark or the admirable "Lebensgeschichte der Blütenpflanzen Mitteleuropas," at present being issued by Drs. Kirchner, Loew, and Schröter.

In a complete survey of the British botany there must be included the successive floras of the earlier geological formations, though they cannot as yet be brought into correlation with the recent or existing floras. In the brilliant progress made recently in this field of study our country and the British Association are worthily represented.

The present provision for the study of the British flora and the means that should be made use of for its extension appear to be these:—

Much excellent work has already been accomplished and put on record towards the investigation of the flora, but much of that store of information is in danger of being overlooked and forgotten or lost, owing to the absence of means to direct attention to where it may be found. A careful revision of what has been done and a systematic subject-index to its stores are urgently required.

The systematic works treating of the flora are in great part not fully representative of the knowledge already possessed, and require to be brought up to date or to be replaced by others.

Great difficulty is caused by the absence of an authoritative synonymic list that would show so far as possible the equivalence of the names employed in the various manuals and lists. There is much reason to wish that uniformity in the use of names of species and varieties should be arrived at, and a representative committee might assist to that end; but, in the meantime, a good synonymic list would be a most helpful step towards relieving a very pressing obstacle to progress.

There is need for a careful analysis of the flora with the view of determining those species that owe their presence here to man's aid, intentional or unconscious; and the inquiry should be directed to ascertain the periods and methods of introduction, any tendencies to become modified in their new homes, their subsequent relations with man, and their influence on the native flora, whether direct or by modifying habitats, as shown by *Lupinus nootkatensis* in the valleys of rivers in Scotland.

Those species that there is reason to regard as not having been introduced by man should be investigated as regards their probable origins and the periods and methods of immigration, evidence from fossil deposits of the period during which they have existed in this country, their constancy or liability to show change during this period, their resemblance to or differences from the types in the countries from which they are believed to have been derived, or the likelihood of their having originated by mutation or by slow change within the British Islands, and their relation to man's influence on them (usually harmful, but occasionally helpful) as affecting their distribution and permanence.

The topographical distribution, though so much has been done in this field during the past sixty or seventy years, still requires careful investigation to determine, not merely that species have been observed in certain districts, but their relative frequency, their relations to man (natives of one part of our country are often aliens in other parts), whether increasing or diminishing, altitudes, habitats, &c. From such a careful topographical survey much should be learned of the conditions that favour or hinder the success of species, of the evolution of new forms and their relation to parent types in distribution, especially in the more isolated districts and islands, and of other biological problems of great interest. A most useful aid towards the preparation of topographical records would be afforded by the issue at a small price of outline maps, so as to allow of a separate map being employed for recording the distribution of each form.

A careful study of the flora is also required from the point of view of structure and development, with comparison of the results obtained here with those of workers in other countries where the same or closely allied species and varieties occur. It is also needed in respect of the relations between the plants and animals of our islands, both as observed here and in comparison with the already extensive records of a similar kind in other countries. On such topics as pollination, distribution of seeds, and injuries inflicted by animals and galls produced by animals or plants we have still to make use very largely of the information gained abroad; and the same holds good with regard to the diseases of plants.

While "English Botany" in its first edition was deservedly regarded as a work of the first rank among floras, it has long been defective as representing our present knowledge of British plants, and it has not been succeeded by any work of nearly equal rank, while other countries now have their great floras of a type in advance of it. There is need for a great work worthy of our country, amply illustrated so as to show, not only the habit of the species and varieties, but also the distinctive characters and the more important biological features of each. Such a flora would probably require to be in the form of monographs by specialists, issued as each could be prepared, but as part of a well-planned whole. It should give for each plant far more than is contained in even the best of our existing British floras. Means of identification must be provided in the description, with emphasised diagnostic characters; but there should also be the necessary synonymy, a summary of topographical distribution, notes on man's influence upon distribution, abundance, &c., on any biological or other point of interest in structure or relations to habitat, environment, associated animals or plants, diseases, &c. Local names, uses, and folklore should also be included; and for this the need is all the greater, because much of such old lore is rapidly being forgotten and tends to be lost. In a national flora there should be included an account of the successive floras of former periods, and, so far as possible, the changes that can be traced in the existing flora from its earliest records to the time of issue should be recorded.

A flora of this kind would not only afford the fullest possible information with regard to the plant world of the British Islands at the date of issue, but would form a standard with which it could be compared at later periods, so as to permit of changes in it being recognised and measured. In the meanwhile, the production of such a flora can be regarded only as an aim towards which to press on, but which cannot be attained until much has been done. But while the fulfilment must be left to others, we can do something to help it on by trying to remove difficulties from the way, and to bring together materials that may be used in its construction.

I have sought to direct attention to the difficulties that I have experienced and to directions in which progress could be made at once, and to provision which should be made for the advancement of the study of the British flora with as little delay as possible. There is, I feel assured, the means of making far more rapid and satisfactory progress towards the goal than has yet been accomplished. Many persons are interested in the subject, and would gladly give their aid if they knew in what way to employ it to the best purpose. As a nation we are apt to trust to individual rather than to combined efforts, and to waste much time and labour in consequence, with discouragement of many who would gladly share the labour in a scheme in which definite parts of the work could be undertaken by them.

I believe that a well-organised botanical survey of the British Islands would give results of great scientific value, and that there is need for it. I believe, also, that means exist to permit of its being carried through. There is no ground to expect that it will be undertaken on the same terms as the Geological Survey. A biological survey must be accomplished by voluntary effort, with possibly some help towards meeting necessary expenses of equipment from funds which are available for assistance in scientific research. Is such a survey not an object fully in accord with the objects for which the British Association exists? In the belief that it is so, I ask you to consider whether such a survey should not be undertaken; and, if you approve the proposal, I further ask that a committee be appointed to report on what steps should be taken towards organising such a survey, and preparing materials for a national flora of the British Islands.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—On Saturday last, October 1, Dr. Mason, the outgoing Vice-Chancellor, announced in his valedictory address to the Senate two munificent benefactions which have recently been offered to the University. The Drapers' Company, which has already done so much for the Agricultural School, has offered a sum of 22,000*l.* towards the cost of erecting a new physiological laboratory on the Downing site, and a further sum of 1000*l.* for fittings. The proposed new laboratory for psychophysics, the cost of which has been collected by Dr. C. S. Myers, will, it is hoped, be erected in the close neighbourhood of the proposed building for physiology.

Since the foundation of the Schröder chair for German, the Cambridge Association has been turning its attention to the further endowment of the teaching of English. Through the instrumentality of Lord Esher, one of their members, Sir Harold Harmsworth became interested in the project, and he has very generously offered to endow a chair of English language and literature by presenting the University with 20,000*l.* The professor is to be called the King Edward VII. professor, and will be elected by the Crown.

The next combined examination for sixty-seven entrance scholarships and a large number of exhibitions at Pembroke, Gonville and Caius, King's, Jesus, Christ's, St. John's, and Emmanuel Colleges will be held on Tuesday, December 6, and following days. Mathematics, classics, and natural sciences will be the subjects of examination at all the above-mentioned colleges. Forms of application for admission to the examination may be obtained at the respective colleges.

THE Child Study Society has arranged a number of lectures and discussions on the recreational activities of children, to be delivered at the Royal Sanitary Institute between now and Christmas. The programme includes the following subjects:—October 13: Some first results of an investigation into the play interests of English elementary-school children, Miss Alice Ravenhill; October 27: games and toys for children under eight, Miss Clara E. Grant; November 3: story of some children's games, Mrs. Lawrence Gomme; November 17: the origin of certain games and toys, Dr. A. C. Haddon, F.R.S.; November 24: philosophy of boys' games, Mr. Felix Clay; December 1: the child's inheritance, Dr. C. W. Saleeby.